The Effect of Housing Vouchers on Crime: Evidence from a Lottery

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Abstract

The Housing Choice Voucher Program (Section 8) is the largest federal housing assistance program; it provides in-kind transfers in the form of rent vouchers to low-income populations. This paper examines the effect of housing voucher receipt on criminal activity. To overcome bias due to selection into the program, we exploit the exogenous variation in lottery-assigned wait-list positions in order to identify the causal effects of the vouchers. Using police department arrest records, we find that voucher receipt increases violent crime, and that this increase is driven by men. We find no effects on arrests for drug or financially motivated crimes.

Keywords: Housing Vouchers, Section 8, Crime, Neighborhood Effects

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1. Introduction

The U.S. government provided $16.6 billion in rent subsidies to disadvantaged families through the Housing Choice Voucher Program in 2013 (Center on Budget and Policy Priorities, 2014). Historically the U.S. government provided housing directly to families in the form of housing projects, though there has been a shift in the last few decades toward housing voucher programs. The federally-funded Housing Choice Voucher Program provides rent support to about 2.1 million households living in non-government housing, which is around 43% of all households receiving federal rental assistance (Center on Budget and Policy Priorities, 2013). The program, often simply called "Section 8," is designed to allow participants to reside in areas previously unaffordable and provide an in-kind transfer to low-income families and individuals. The program is means-tested, and participating families receive a rent subsidy that is paid directly to their landlords.

In this paper, we examine the effect of Section 8 vouchers on crime. Vouchers could affect crime through two major channels: income transfer effects and neighborhood effects. Income transfers can relieve financial pressures that could otherwise cause recipients to seek illicit income. Alternatively, income transfers could also provide the funds or leisure time necessary to participate in illegal activities. Voucher receipt could also affect criminal involvement by changing neighborhood influences. Moving to a better neighborhood could reduce crime via positive peer effects or social norms, or it could increase crime by providing easier and wealthier targets.

Understanding the causal effects of housing mobility programs is challenging because individuals select to participate in voucher programs. Eligible families that opt to use vouchers may also take other steps to better their lives, creating a substantial source of selection bias. Many studies of voucher programs rely on randomized social experiments, such as the Moving to Opportunity (MTO) experiment. Often, Section 8 housing vouchers are given out via randomized lottery because it is not an entitlement program and there are usually more applicants than vouchers. Some papers rely on this random variation in voucher allocation for identification.[1]

[1] Others have used the Gatreux Program (a precursor of MTO, Popkin et al., 1993), random assignment into public housing (Oreopoulos, 2003) or Hurricane Katrina (Hussey et al., 2011, Kirk, 2012) to study mobility and crime.

In this paper, we exploit the exogenous variation in randomized waitlist positions assigned using a lottery in order to identify the causal effects of Section 8 vouchers on arrests of adult household heads. The lottery we study was administered by the housing authority of the City of Houston. We link the voucher recipients to arrest records from the Houston Police Department (HPD) to determine whether voucher receipt has an effect on arrests for various types of crimes. We estimate the effects using intent-to-treat models identified using the timing of voucher receipt, which is determined by the randomized lottery.

To support the assumption that waitlist positions are indeed random and that there are no differences between those who lease-up with a voucher earlier and those who lease-up later, we perform empirical tests for differences in pre-lottery characteristics of applicants. The relationships between pre-lottery characteristics and waitlist positions are consistent with waitlist randomization and that the type of individuals who lease-up at different times are no different. Because MTO studies have consistently found asymmetric effects by gender (Katz et al., 2001, Clampet-Lundquist et al., 2011, Jacob et al., 2014, Ludwig and Kling, 2007, Kling et al., 2005, and Kling et al., 2007), we also test for effects of the voucher within gender subgroups.

Results indicate that some criminal outcomes actually increase while others remain unchanged due to voucher receipt. We find that the probability of being arrested for a violent offense in a quarter increases by 0.066 percentage points (a nearly 95% increase) and that the effect is primarily driven by men for whom probability of arrest increases by more than two-fold. Our results highlight an unintended consequence of the Section 8 Housing Voucher Program – an increase in arrests for violent crime. We attribute this increase to the additional funds and leisure time available to voucher recipients that can be used to commit crimes; both of these mechanisms have been shown to increase illegal activity previously (Dobkin and Puller, 2007, Riddell and Riddell, 2005, Foley, 2011, and Lin, 2008).

Our contribution to the literature is three-fold. The primary contribution is that we are the first to consider the effect of housing vouchers on criminal outcomes for adult recipients using a randomized lottery.[2] We join an extensive crime literature produced by

[2] Leech (2013) uses NLSY data to study the relationship between voucher receipt on self-reported violent altercations for young adult heads of household receiving vouchers. She suggests that selection bias is a methodological shortcoming of her study. She finds that voucher receipt is associated with reduced violent altercations, but that this association is not present in the subsample of black recipients.

MTO, which, with the exception of Ludwig and Kling (2007) who studied the contagion effects of neighborhood crime on both adults and juveniles, primarily focuses on outcomes for youth whose families received vouchers. While most of these studies have found that MTO caused positive or neutral effects for female youth, their findings for male youth have been surprisingly negative (Clampet-Lundquist et al., 2011, Kling et al., 2005, Sciandra et al., 2013, and Zuberi, 2012). The only exception is Katz et al. (2001), who shows that male youth have less behavior problems after moving through MTO. The effect of Section 8 voucher receipt on adult criminal outcomes is yet to be documented although Jacob, Kapustin and Ludwig (2014) use a lottery-based identification strategy to show that there is no effect on arrest rates of juveniles whose families received vouchers (among other outcomes).

Secondly, we study the impact of residential mobility in the context of the Section 8 voucher program which accounts for a significant portion of federal housing assistance (43% according to the Center on Budget and Policy Priorities, 2013). Hence, our results are relevant for predicting the impact of Section 8 in other contexts. Again, we are the first to consider the effects of Section 8 voucher receipt on adult criminal outcomes using a lottery, so the policy implications of our results are quite significant.

Finally, our results speak to the relative impact of neighborhood and income effects that arise due to voucher receipt. We provide new evidence that the neighborhoods into which recipients move are only slightly different from their pre-voucher neighborhoods along demographic and economic grounds. This result is in agreement with existing literature on Section 8 vouchers (Jacob and Ludwig, 2012, and Lens, 2013) and suggests that the effect of the income transfers maybe be the larger influence. We also believe that income transfers are the primary mechanism because the increase in crimes that we detect is in line with the negative outcomes found in the previous literature on government cash transfer programs. (Dobkin and Puller, 2007, Kenkel et al., 2014, Riddell and Riddell, 2005, Evans and Moore, 2011, and Foley, 2011).

Additional income can also affect crime by altering recipients' employment decisions in that it may afford recipients the opportunity to take additional leisure time, which they could use to participate in crime, among other things. Empirically, Section 8 voucher receipt does, in fact, cause lower labor force participation rates and earnings (Jacob and Ludwig, 2012, Carlson et al., 2012), and a similar effect has been detected for food stamps (Hoynes and Schanzenbach, 2012).

Overall, our study documents an unintended consequence of Section 8 housing vouchers (an increase in arrests for violent crime for adult heads of household). The program is the largest housing assistance program in the U.S., so this repercussion could be quite large on a national scale. The disparity between findings for males and females implies that large income shocks have heterogeneous effects on recipients by gender and has policy implications for screening and oversight within the voucher program.

2. Background

The Section 8 Housing Voucher program is the largest housing assistance program in the U.S. The vouchers are federally-funded, and the U.S Department of Housing and Urban Development (HUD) allocates the funds to local housing authorities and sets eligibility standards across the nation. HUD requires that participants' incomes fall below 80% of the median family income in the area, adjusting for family size, and stipulates that seventy-five percent of new voucher recipients' incomes are less than 30% of the local median family income (Center on Budget and Policy Priorities, 2013). Voucher recipients must also be citizens or of other eligible immigration status, and the Houston Housing Authority (HHA) can deny eligibility for drug-related criminal activity (Houston Housing Authority, 2013). Local housing authorities submit the subsidies directly to the recipients' new landlords. Continued eligibility is assessed annually, and recipients are allowed to use their vouchers in any U.S. city with the Housing Choice Voucher Program in place, although, according to HHA, less than 10% of voucher recipients move to a different city.

HHA serves around 60,000 Houstonians, over 80% of whom are participants in the Housing Choice Voucher Program. HHA accepted voucher applications from December 11, 2006, to December 27, 2006, and received over 29,000 applications. All applicants were assigned a lottery number regardless of whether they met the eligibility criteria. Vouchers were then extended to the applicants as the funding became available starting with the lowest lottery numbers. The lottery and voucher service processes are outlined in Figure 1. Once an applicant's wait-list position was reached, he or she received a voucher screening packet from HHA and the verification process began. After their eligibility was verified, families were required to sign a lease in a Section 8 approved community in order to participate in the program. The average time between HHA sending the initial packet and the recipient leasing up with the voucher was 6 months. Because the speed of this process varied by applicant, the vouchers were not issued in perfect sequential order.[3]

The first vouchers were issued in July 2007. However, the majority of vouchers were serviced starting in 2009, and HHA had sent screening packets to almost all the lottery numbers by October 2012. Overall, take-up rate was about 23%. The low take up is a result of applicants dropping out at every step of the voucher service process. Based on the last known application statuses, close to 60% of the verification packets were not returned to HHA by the families. 2.5% of the applicants were found to be ineligible after verification and about 4% of them were unable to sign a lease in time, and the voucher expired.

We geocode the addresses provided on the applications and the addresses of current residents in order to describe the pre and post lottery neighborhoods of voucher recipients. Figure 2 shows the density of these two types of addresses across the city using heat maps, and contains the boundaries of HPD's police districts.[4] The distribution of addresses indicates that the voucher-users are not moving to different parts of the city on the whole. Changes in neighborhood (defined as Census tract and police division) experienced by the voucher recipients are documented in Table 1. On average, recipients moved 4.7 miles and the voucher paid $628 toward rent every month. Only 3.4% of these recipients were living in public housing at the time of application. Differences between the neighborhoods before and after the lottery are listed in Panel B. We report median rent in 2012 from the American Community Survey, and we see that voucher recipients move to Census Tracts with only $40 higher monthly median rent. We report demographics and socioeconomic characteristics of the census tracts from the 2010 census and crime rates from 2000-2005 for the police divisions. The post-lottery neighborhoods are somewhat better off in terms of parameters such as unemployment rate, household income, poverty rate and crime rates.

[3] In addition, some lottery numbers were called too far out of order for this to be the case. HHA says that there were no priority groups in the lottery, and there are no common characteristics of these applicants who were called out of sequence. However, because we use the assigned lottery number to predict voucher service, our estimates should be unbiased by the occasional non-sequential calling of lottery numbers.

[4] The heat maps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell.

These differences in neighborhoods are minimal; for example, voucher use neighborhoods had on average 2.1 less crimes per year per 1000 residents, which is a 1.5 percent improvement. As a result, we believe that any impact of the vouchers in this context can be most reasonably attributed to the income shock induced by an annual rent subsidy of more than $7,500 on average. Additional income, itself, can be spent on things that can increase or decrease the likelihood of arrest. It could also alleviate financial pressures, which would reduce the recipients' motivations to be involved in crime that can lead to financial gain, such as selling illegal drugs or theft. The net effect is ambiguous, and the question will ultimately have to be answered empirically. The theoretical implications of an in-kind transfer on labor decisions are similarly ambiguous because they depend on the shape of each recipient's indifference curves. However, researchers find that vouchers reduce earnings and labor force participation (Jacob and Ludwig, 2012). Like additional income, additional leisure time can be put toward things that either increase or decrease the likelihood of arrest.

Given that much of the existing literature has examined MTO, it is important to highlight the differences between the two housing programs. MTO researchers recruited only public housing residents to participate and split them into 3 groups. The first (the "MTO experimental group") received vouchers and was only allowed to use them in census tracts with low poverty rates. The second was simply given vouchers and called the "Section 8 experimental group" because their treatment was similar to Section 8. The third was a control. The neighborhoods into which MTO experimental families moved were notably different from the ones that they left (Katz et al., 2001, and Kling et al., 2005). The MTO Section 8 experimental group moved to areas more like their neighborhoods of origin than the MTO experimental group (Kling et al., 2005), although there was some improvement. Similar to findings for the MTO Section 8 group and Jacob and Ludwig's findings (2012), we find that Census tract characteristics of new neighborhoods are slightly improved, but the changes are not large. Additionally, the neighborhood changes we detect are smaller in relative terms than those found in MTO studies for the MTO experimental group. For example, HHA voucher recipients moved to neighborhoods with a 7.6% lower average poverty rate, while MTO experimental group participants moved to neighborhoods with a 26% lower average poverty rate (Kling et al., 2007).

MTO's driving mechanisms were also different because it targeted families living in public housing. MTO required the families to move and provided little, if any, additional financial gains directly for the families. Section 8, on the other hand, provides a substantial income transfer, and HUD does not allow local housing authorities to place restrictions on neighborhoods in which recipients can use vouchers. While we don't have any information on the Section 8 participants' reasons for applying for the program, it is well documented that MTO families cite a desire to get away from gangs and drugs as the main reason for volunteering (e.g. Kling et al., 2005). This concern is likely addressed by the neighborhood change facilitated by MTO, but Section 8 voucher receipt may have little effect on this. The populations opting into these two programs are also likely to be quite different due to incongruous motivations.

3. Data

The Houston Housing Authority provided us with information on the voucher applicants. These confidential data include lottery numbers, the number of bedrooms needed (calculated based on family size), the date on which HHA sent the voucher screening packet and the move-in date for voucher recipients. The data also include name and birthdate, which we use to match the HHA data to arrest records. They also provided additional, more detailed information on the set of applicants who are current participants in Housing Choice Voucher Program. For this group, we also know their race and homeless status at the time of admission.

HHA assigned lottery numbers up to 29,327, but we limit our sample to those living in Houston at the time of application. Additionally, there are a small number of duplicate applicants; we assign them their lowest lottery number. We also drop applicants with lottery numbers over 24,000 because the take up rate is much lower among the later lottery numbers indicating a probable change in the voucher service process after that point.

Additionally, we restrict our analysis to those applicants who eventually leased-up with a voucher. Estimates from the sample unconditional on take-up are of similar magnitudes as those from the sample conditional on take-up, but are measured imprecisely given the relatively low take-up rates in Houston. The take-up rate is only 23%, which is low relative to the 69% national average estimated by Finkel and Burton (2001). We also perform empirical tests, detailed in the following section, to support the assumption that the population of early movers is no different from that of late movers. The resulting sample size is 4,510.

Treatment is leasing-up using a voucher. Intuitively, the "voucher service" quarter (intent-to-treat) is the quarter during which the applicant would have leased-up according to lottery number. On average, recipients take approximately 6 months to complete the screening process and actually relocate using the voucher. We determine whether the individual has been sent a screening packet by a given quarter based on his or her lottery number relative to the numbers called by that point.[5] Lagging this by two quarters gives us the "voucher service" quarter.

Table 2 reports pre-lottery descriptive statistics. We report them for the population of voucher-users, and we show them separately by low and high lottery numbers (applicants whose vouchers were serviced earliest and those applicants whose vouchers were serviced latest) to show similarity between these groups prior to the lottery. If these groups are different on important measures, it could indicate that HHA gave preference to some groups in lottery number assignment or that the type of individual who leased-up with a voucher changed over time.

The average voucher recipient was around 35 years old at the time of application and required just over two bedrooms (indicating that the average family size was between 2 and 6, Housing Choice Voucher Program Guidebook, 2001). Around 94% of recipients are black, and using 2012 voting records from the Harris County Tax Assessor's office, we estimate that nearly 90% of applicants are female.[6] Less than 1% of recipients were homeless at the time of application. The number of observations varies for race and homeless status because they are only available for current HHA voucher recipients. There is only one statistically significant difference between the high and low lottery numbers on any of these measures (number of bedrooms required), and it is not economically significant.

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[5] Since the lottery numbers were not called in perfect sequential order, we cannot identify the range of lottery numbers simply using the smallest and largest lottery number called in a quarter. Additionally, for approximately 5,000 applicants, there is no recorded screening packet issue date. As a workaround, within each quarter from 2007 to 2011, we take the lottery number at the 75th percentile to be the last number called in that quarter. We assign the next lottery number as the first number called in the subsequent quarter.

[6] We calculate the percentage of Harris County voters whose reported gender is "male" for each unique first name in the list of registered voters. If there are more than 4 individuals with a given name, and 70% or more are listed as males, the name is assigned the gender "male." If 30% or less are listed as male, we classify the name as "female." Applicants with unmatched or ambiguous names are omitted from subgroup analysis.

We match the HHA data to arrest records provided by the Houston Police Department (HPD). The arrest records are reported at the time of booking and include information on the offense as well as the arrestee's name, birthdate and reported home address. We match the HHA and HPD data using name and birthdate, and we perform secondary matches using the Levenshtein distance and soundex code of each name for unmatched records.[7] The arrest records range from January 1990 to November 2011 and we use the matched arrest records to create measures of criminal activity in the period before the lottery and a quarterly panel of arrests for the study period after voucher service commenced (from quarter 1 of 2007 to quarter 3 of 2011).

We consider arrests of any type and specifically categorize violent offenses, drug offenses and financially-motivated offenses.[8] We measure arrests as a binary indicator for whether the recipient was arrested. The pre-lottery crime measures are constructed for the 5 years prior to the lottery, and we create an additional binary indicator for whether the applicant was arrested at least once between 1990 and 2006. Around 20% of applicants were arrested during that 16 year period, and approximately 9% of applicants had been arrested in the 5 years prior to the lottery. There are no statistically significant differences between high and low lottery number individuals.

Using the geocoded application addresses, we find that voucher recipients lived in census tracts with around 51% black residents, and around 36% Hispanic residents. The mean unemployment rate was around 12% and the mean of median family income was just approximately $34,000. The mean poverty rate was quite high at over 30%. Voucher recipients with higher lottery numbers lived in census tracts with slightly higher unemployment rates and slightly lower poverty rates. Voucher recipients lived in police divisions with an annual average of 135 crimes per 1000 residents. On average, nearly 60 of these crimes were property crimes and only were 13 were violent. Recipients with higher lottery numbers lived in neighborhoods with 1.1 more crimes per year per 1000 residents, a marginal difference considering the average crime rate. Although some of these difference are statistically significant, none of them are economically significant. The similarity between these groups indicates that pre-lottery characteristics are

[7] For the arrest records that are unmatched by name and birthdate, we calculate the Levenshtein distance for the first and last names, if the sum of the Levenshtein distances is less than 3, conditional on an exact birthdate match, we accept the match. For the records that are still unmatched, we perform an exact soundex code match.

[8] A complete list of all offenses and crime categories are provided in Appendix Table A1.

distributed randomly across lottery numbers and suggests that the lottery was in fact random.

In Table 3, we report post-lottery descriptive statistics. The purpose of this table is to preview results in a cross-sectional manner. We show measures of program take-up (whether the individual's voucher has been serviced and whether he or she has leased-up by a quarter) as well as all of the arrest outcomes averaged over person-quarters (from quarter 1 of 2010 to quarter 3 of 2011). Statistics are restricted to the last year of the panel, when vouchers for the low lottery numbers had mostly been serviced, but it was not so for the high lottery numbers. Specifically, for individuals with lower lottery numbers (below the median) their vouchers had been serviced for, on average, 89% of person-quarters. Conversely, the vouchers of those with high numbers had been serviced for around 17% of person-quarters during this period. Lease-up follows a similar pattern where low lottery numbers are nearly 70 percentage points more likely to have leased up during a person-quarter. The post-lottery statistics for the outcomes – probability of arrest in a person-quarter for different crime categories – indicate that recipients with low lottery numbers are significantly more likely to be arrested for crimes of any type and violent crimes in this period.

4. Identification and Methods

In this study, we identify the effect of housing vouchers on criminal involvement using a lottery. The lottery randomized the order of the waitlist from which applicants were called for voucher service and actual voucher receipt. This randomization allows us to identify the causal effects of voucher receipt. Because the random variation we exploit for identification is in timing, we analyze criminal outcomes using a quarterly panel of arrests using pooled cross-sectional models.

Because we consider the group of applicants who eventually lease-up with a voucher, our identifying assumption is that timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that individuals who lease up later with a voucher had similar propensities to commit crime as those who leased up earlier. We condition on lease-up because the take-up rate is particularly low for this lottery, resulting in imprecise estimates for the entire sample. Because take-up rates are consistent across time, we believe that the early and later leasers are no different, and we show results from additional empirical tests to support this in the following section.

Before we estimate intent-to-treat effects of the vouchers, we first examine evidence on whether the randomization was properly implemented and whether early movers are different from late movers. We test this empirically by examining the extent to which demographic and criminal history variables are correlated with lottery number or voucher service quarter. We represent this graphically by simply plotting these characteristics against lottery number and estimate it empirically according to the following equation:

$$control_i = \alpha + \beta \, voucher \, order_i + u_i \quad (1)$$

In the above equation, *voucher order*$_i$ is either the randomized lottery number assigned to applicant *i* or his/her voucher service quarter (where the first quarter of 2007 is indexed to one). We test each applicant's age at the time of lottery, number of bedrooms, and the set of criminal history variables: whether (and how many times) the applicant was arrested in the 5 years prior for any type of offense, a violent offense, a drug offense, or a financially-motivated offense, and whether the applicant was ever arrested between 1990 and 2006. For the applicants who are current residents, we also look for correlations in race and homelessness status at time of admission, and gender. Similarly, for the applicants whose addresses were geocoded successfully, we check for a relationship between voucher service order and neighborhood characteristics prior to the lottery.

To estimate the impact of Section 8 vouchers on arrests, we estimate the intent-to-treat effect of voucher service. We estimate regressions of the following form:

$$outcome_{it} = \rho + \pi \, post \, voucher \, service_{it} + \Psi X_i + \phi_t + \varepsilon_{it} \quad (2)$$

In the above equation, *post voucher service*$_{it}$ is a dummy variable equal to one if individual *i*'s voucher has been serviced by quarter *t*. The results should be interpreted as the effects of potential voucher use based on lottery number, and can be reweighted by the first stage to recover a local average treatment effect. To estimate this first stage, we use an indicator for whether individual *i* had leased up using a voucher by quarter *t*, called *post lease-up*$_{it}$, as the outcome variable.

We estimate the intent-to-treat effects using a number of recidivism outcomes: whether an individual was arrested for crimes of any type, violent crimes, financially-motived crimes, and drug crimes in quarter *t*.

We estimate all models using quarter fixed effects as well as robust standard errors that are clustered at the individual level. All specifications are estimated both with and without controls for past crime (probability of arrest for the particular crime category in the 5 years prior to the lottery), age at the time of the lottery and a proxy for family size (number of bedrooms); this tests whether timing of voucher service is correlated with any of the observable characteristics.[9] If specifications that do and do not include controls have similar estimates, this can be interpreted as evidence that is consistent with randomization of timing of lease-up. We also replicate the main results using a negative binomial model to show that results are not sensitive to the parametric specification imposed by the linear probability model.

We estimate all of the above models for all heads of household, as well as for men and women, separately, because there is considerable evidence in the literature that they respond differently to mobility programs (e.g. Clampet-Lundquist et al., 2011, Katz et al., 2001, Kling et al., 2005). We also take a cue from the existing mobility literature and explore the possibility of dynamic effects over time (Kling et al., 2005). Specifically, we estimate separate treatment effects for the first year after voucher service and later years of voucher service by using two binary treatment variables. The first is equal to one if the applicant's voucher had been serviced within the past year, and the second is equal to one if the applicant's voucher had been serviced more than a year ago. Intent-to-treat estimates are reported for this specification for the overall population and men and women separately.

5. Results

5.1 Tests of Identifying Assumptions

Identification of the model comes from the assumption that the timing of voucher receipt among those who eventually received the voucher was exogenous. That is, we assume that individuals who lease up later with a voucher had similar propensities to commit crime as those who leased up earlier. Because the timing of voucher packet issue and therefore subsequent move into subsidized housing was determined by a randomized

[9] We perform additional analyses controlling for application address census tract characteristics and police division crime statistics in Appendix Table A3 because they are not available for all recipients.

lottery, this is a reasonable assumption. Nevertheless, we test this assumption empirically in several ways.

First, we test this by showing that take-up rates did not change over time. If the rate had changed as HHA serviced higher lottery numbers, it could indicate that late movers may be different from the early movers. Figure 3 plots take-up rates over lottery numbers, and we also separate this by gender in Figure 4. Take-up rates do not appear to change over the range of lottery numbers. We also test this empirically to determine whether there is a correlation between lottery number and take-up. We report estimates of this correlation within the figures, and there is not a statistically significant relationship for all applicants or for males and females separately.

Second, we test for correlations between observable characteristics and both lottery number and voucher service quarter. If the identifying assumption holds, we expect to see no correlations between these measures and demographic variables or criminal history measures. For example, if the most motivated applicants were assigned lower numbers through manipulation of the lottery mechanism, we would see a negative correlation between lottery number and indicators of stability such as age, gender, and criminal history. Conversely, if only the most stable individuals move in later because they are less likely to move, we would see a positive correlation.

Figures 5 and 6 represent these relationships graphically for criminal history (probability of past arrests, past violent arrests, past drug arrests and past financial arrests) and demographic (age and number of bedrooms) variables for male and female recipients, respectively. Each dot is a local average for a bin of lottery numbers. If lottery number is truly random and the "mover" population is constant over time in observable characteristics, the local averages should exhibit a flat relationship. This does appear to be the case, and we take this as support for the identification assumption.

Table 4 reports the results of the empirical tests. Column 1 contains the results from 24 separate regressions using lottery number as the independent variable as described by equation (1). Similarly, the regressions that generated column 2 all use indexed voucher service quarter as the independent variable. Each row is labeled for the covariate used as the dependent variable.

There is only one statistically significant correlation between individual characteristics and voucher order. This effect is on the number of bedrooms, but it is not economically significant. It predicts that the individual with the highest lottery number, 24,000, would require 0.11 more bedrooms than the individual with the lowest lottery number. There are no significant relationships between lottery number or voucher service quarter and criminal histories (perhaps the most important determinants of future arrests).

There are a few significant correlations between voucher order and neighborhood characteristics, but none of them are economically significant. The higher lottery numbers come from census tracts with higher unemployment and lower poverty rates. The higher lottery numbers also come from police divisions with higher crimes rates overall and for violent crimes. Again, none of these differences are economically significant. For example, if we consider 2 applicants whose vouchers were serviced 2 years apart, we would expect the later-served applicant's original neighborhood to only have 3.25 (2% of the mean) additional crimes per 1000 population annually. Importantly, because we find an increase in violent crime arrests for recipients, if we assume recipients from low crime neighborhoods have a lower propensity for crime, any indication that earlier movers came from better neighborhoods would imply that our findings are a lower bound of the true increase. As an additional check, we also estimate the main models with and without these controls and show that the results are invariant, indicating that timing of voucher service is orthogonal to these characteristics.

5.2 Effect of Voucher Service on Lease-Up

Before examining the effect of voucher receipt on criminal outcomes, we first document that the voucher recipients are likely to lease-up when we predict that their vouchers were serviced. Our ability to use lottery variation to identify effects hinges on the extent to which the lottery predicts lease-up.

Table 5 contains the first stage results obtained by estimating equation (2) using *post lease-up* as the outcome. The table reports the coefficient on *post voucher service* from 4 separate regressions. The first two columns indicate that in 84.9% of the person-quarters after voucher service, the voucher recipient had previously leased-up. This coefficient is identical when we include controls in column 2, suggesting that controls are orthogonal to *post voucher service*. Columns 3 and 4 indicate that *post voucher service* is equally predictive of lease-up for men and women. The large magnitude of the first stage results means that the intent-to-treat estimates will be very close to the local average treatment effects.

5.3 Effect of Voucher Receipt on Arrests

Table 6 contains the main results for the full sample of voucher recipients, as well as for men and women separately. We estimate equation (2) to measure the intent-to-treat using both ordinary least squares and a negative binomial model. We also report the mean of each outcome variable from the year preceding the lottery (2006) for the relevant population; we refer to it as the "pre-lottery mean." Each row is labeled for the outcome variable for which the results are generated. We also run models both with and without controls and demonstrate that our results are unresponsive to their inclusion, indicating that the timing of voucher service is unrelated to these observable characteristics and, we expect, unobservable characteristics.[10]

Results show no evidence that voucher service and lease-up affect arrests for all types of crimes combined. All of the coefficients are statistically insignificant. When we run the models separately for males and females, we find that the coefficients are all negative and statistically insignificant.

We also look at arrests for specific types of crimes that are likely to be affected by voucher receipt: violent crimes, financially-motivated crimes, and drug crimes. For the overall population, there are only statistically significant effects for violent crimes.

Results indicate that there are considerable differences in effects across gender, and that this overall effect on violent crime arrests is mostly driven by males. The magnitude of said effect indicates that voucher receipt increases quarterly probability of violent crime arrest by 0.066 percentage points. This is a nearly 95% increase. The point estimate for males is large at 0.38 percentage points and is statistically significant. If the voucher is given to a 100 men, the number of men arrested for violent offences in a quarter increases from 1.3 to 4.1, which roughly translates to 15 more arrests in a year. The point estimates for females are close to zero and negative, leading us to attribute this effect primarily to males.

Negative binomial results for violent crime are similarly large and statistically significant. For the overall population, results indicate around a 78% increase in violent crime arrests.

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[10] Table 6 contains models that include controls observed for the entire sample. We also rerun the main models using neighborhood controls only available for a subset of recipients. Results are not statistically different from those here, the effect on violent crimes remains statistically significant (the coefficient is 0.00381 compared to 0.00384) and coefficients change minimally between models with and without controls. Results are in Appendix Table A3.

Similar to the linear probability models, this effect is larger for males and statistically significant.

Drug crime arrests appear to be unaffected by voucher receipt. Effects for males and females combined as well as separately are all statistically indistinguishable from zero. We do find evidence that males are arrested for more drug crimes in the 6 months during which their eligibility verification and voucher process is underway but they have not yet moved (Appendix Table A2). This approximately 16% increase is the effect of an impending income shock and can be interpreted as an announcement effect. Financially-motivated crime arrests appear to be unaffected by voucher receipt overall and for women. The coefficients are negative and large for men, but are not statistically distinguishable from zero. We attribute the lack of significance to limited statistical power given the small sample size.

Results show little evidence that vouchers affect crime for women. For all crime subtypes explored, the coefficients for females are orders of magnitude smaller than those for males, and many are also small relative to the pre-lottery means.

As discussed earlier, in addition to expecting differential effects by gender, one might also expect differential effects by how long an individual has been treated (as Kling et al., 2005, found for juveniles). Table 7 contains the results from models that allow for the effect of voucher service to vary over time. Specifically, we estimate effects of two different intent-to-treat measures: whether the applicant's voucher was serviced within the last year, and whether the applicant's voucher was serviced more than a year ago. Because the bulk of vouchers were serviced in 2009 or later and our panel ends in 2011, most applicants were treated for just over 2 years or less. Because ordinary least squares results and negative binomial results are so similar for the main results, we estimate these models using just ordinary least squares for simplicity.

Panels A to D contain results from different crime categories. Column 1 reports coefficients for the overall population, and similar to results reported previously, there is little evidence of an overall effect for all arrests, drug arrests and financially-motivated arrests. Violent arrests are most responsive to voucher receipt during the first year of voucher use. For females, there is little evidence that applicants' responses to voucher service change over treatment duration; no estimates for either duration are significant at any level. However, results for males show that the effects described in Table 6 are greater in the quarters within a year of voucher service. The coefficients for violent arrests are generally large and statistically significant for those quarters, although they are close in magnitude to the coefficients for later quarters.

In summary, we find that voucher receipt causes a rather large increase in violent crime arrests for recipients, and the increase is driven by male heads of household. Additionally, the increase seems to be the most pronounced in the first year after voucher receipt. We find that the vouchers have no effect on female heads of household or on other types of crime. There does seem to be an announcement effect for drug crime that indicates that male heads of household are arrested for more drug crimes during the voucher processing period.

5.4 Attrition Test

One potential concern for our study is attrition. That is, to the extent that individuals with low lottery numbers are more or less likely to move out of Houston than individuals with high numbers, our results could be biased. For example, if individuals who receive high lottery numbers are more likely to leave Houston and commit crimes elsewhere that are not measured in our data, then our results could overstate the increase in violent crime due to housing vouchers.

We empirically test whether applicants with lower lottery numbers and earlier voucher service quarters are more or less likely to have stayed in Houston than those with higher numbers and later voucher service quarters. We proxy for continued Houston residence with whether the applicant was registered to vote in the City of Houston in 2012 and whether he or she voted in the 2012 general election. Specifically, we estimate an analog of equation (1) used in the test of randomization, to test for a relationship between when an applicant's voucher was serviced and whether he or she stayed in the city.

We show the raw data in Figure 7; it plots voter registration and actual voting in 2012 against lottery numbers. Each dot represents a local average for a bin of about 50 males' or about 150 females' lottery numbers. There is no discernable correlation between lottery number and either voting outcome. This suggests that individuals whose numbers were called early in the sample period were no more or less likely to be in Houston several years later than those whose numbers were called late in the sample period.

Table 8 contains the results of the empirical test. In the odd columns the dependent variable is a dummy for being registered in 2012, and in the even columns it is a dummy for voting in 2012. There are no significant correlations between when an applicant was served by HHA (measured by lottery number and voucher service quarter) and the two voting outcomes. We test for differential attrition for males and females separately because the significant results discussed in the previous section were gender specific. There is no evidence of differential attrition for males or females.

6. Conclusions

In this study, we analyze whether receiving a housing voucher affects criminal activity for low income individuals. The timing of voucher receipt was determined by an individual's position on the wait-list, which was assigned using a randomized lottery. We use the lottery numbers to determine by when an individual's wait-list number was serviced and estimate intent-to-treat models to determine the effect on arrests overall and arrests for types of crimes likely to be affected by voucher receipt.

Results indicate that voucher receipt causes a large increase in violent crime arrests for male recipients. They do not, however, indicate that vouchers have an effect on women or on other types of crime. Specifically, we find a statistically significant increase in violent crime arrests for the overall population and male recipients alone. There are no statistically significant effects for female recipients alone. This dichotomy in the effects for male and female housing voucher recipients is consistent with previous research on the effect of the MTO experiment on juvenile criminal outcomes (Kling et al., 2005, Sciandra et al., 2013, Zuberi, 2012, and Clampet-Lundquist et al., 2011).

Although the Housing Choice Voucher Program was designed to facilitate mobility in addition to providing an in-kind transfer to low-income individuals, we show that the neighborhoods into which recipients move are only slightly less disadvantaged from their original neighborhoods. Again, this finding is consistent with previous research (Lens et al., 2013). The lack of a meaningful change in neighborhood leads us to believe that the massive income transfer provided to recipients is driving the increase in violent crime that we detect.

Such an income transfer could work to either increase or decrease arrests for recipients depending on how they choose to spend their additional income and how they change their labor decisions. Based on the increase in violent crime arrests that we detect for males we believe that males in our sample may be spending the extra income on things that lead to violent crime such as drugs and alcohol, which is a well-supported outcome in the government transfer literature (Dobkin and Puller, 2007, and Riddell and Riddell, 2005). Because Jacob and Ludwig show that Section 8 voucher recipients work less hours (2012), we also believe that additional leisure time contributes to this negative consequence as it affords recipients more time to socialize. If that socialization also includes drugs and alcohol, this is even more likely to be the case.

Our results suggest that housing vouchers may have unintended consequences for some recipients, which is an important consideration in discussions of the future of housing assistance programs. We provide evidence that large income shocks have heterogeneous effects on recipients, particularly by gender.

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Table 1: Comparison of Application and Voucher Use Addresses for Movers

Panel A: Voucher Use Characteristics	**Mean (s.d.)**
Distance moved in miles	4.7 (5.5)
Rent paid by voucher	628 (253)
Rent paid by resident	205 (203)
Percent living in public housing before	3.4 (0.2)
Observations	1693

Panel B: Neighborhood Characteristics	**Application Address**	**Voucher Use Address**	**Difference**
Census Tract Characteristics			
Median age	31.7 (4.8)	30.7 (4.5)	-1.0*** (0.2)
Percent over 18 years	70.7 (5.0)	69.7 (4.8)	-1.0*** (0.2)
Percent male	48.0 (3.1)	47.9 (3.0)	-0.1 (0.1)
Percent white	26.5 (18.0)	30.1 (17.9)	3.6*** (0.6)
Percent black	52.5 (27.1)	47.1 (26.4)	-5.4*** (0.9)
Percent Hispanic	35.4 (21.4)	37.9 (21.0)	2.5*** (0.7)
Median rent	797 (168)	836 (181)	39*** (6)
Percent housing occupied	86.9 (7.3)	87.7 (7.0)	0.8*** (0.2)
Percent unemployment	12.3 (5.6)	11.1 (5.4)	-1.2*** (0.2)
Median household income	33213 (12329)	35727 (13505)	2514*** (444)
Median family income	37637 (14950)	39446 (14791)	1809*** (511)
Percent below poverty	34.6 (15.9)	32 (16.0)	-2.6*** (0.5)
Observations	1693	1693	
Police Division Characteristics (Annual rates per 1000 population)			
Crime rate	135.9 (23.3)	133.8 (25)	-2.1** (0.8)
Murder rate	0.2 (0.0)	0.2 (0.0)	0.0 (0.0)
Violent crime rate	13.5 (3.0)	13.2 (3.4)	-0.3*** (0.1)
Property crime rate	58.9 (10.8)	58.5 (11.0)	-0.4 (0.4)
Observations	1389	1176	

Notes: Statistics are shown for voucher recipients for whom both pre and post-lottery addresses were available and geocodable. Crime rates at the police division level are from 2000 to 2005.
Significance: * 10% level; ** 5% level; *** 1% level

Table 2: Pre-Lottery Descriptive Statistics

	All			Low Lottery Numbers	High Lottery Numbers	Difference
	Observations	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
Lottery Variables						
Lottery number	4510	11852 (6734)	8 - 23980	6078 (3422)	17625 (3507)	-11547*** (103)
Voucher service quarter	4510	12.9 (3.3)	8 - 17	10.0 (2.2)	15.8 (0.7)	-5.8*** (0.0)
HHH Characterestics						
Age (in years)	4510	35.3 (14.2)	16 - 97	35.1 (14.2)	35.5 (14.1)	-0.4 (0.4)
Number of bedrooms	4510	2.20 (0.96)	1 - 8	2.17 (0.93)	2.23 (0.98)	-0.06** (0.03)
Male	3844	0.12 (0.29)	0 - 1	0.12 (0.30)	0.11 (0.28)	0.01 (0.01)
Black	2612	0.94 (0.24)	0 - 1	0.94 (0.24)	0.94 (0.23)	0.00 (0.01)
White	2612	0.03 (0.18)	0 - 1	0.03 (0.18)	0.03 (0.18)	0.00 (0.01)
Other race	2612	0.03 (0.16)	0 - 1	0.03 (0.17)	0.02 (0.15)	0.01 (0.01)
Homeless at the time of admission	2612	0.00 (0.03)	0 - 1	0.00 (0.04)	0.00 (0.03)	0.00 (0.00)
Arrested in 5 years prior to lottery	4510	0.09 (0.28)	0 - 1	0.09 (0.29)	0.08 (0.28)	0.01 (0.01)
Violent offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.12)	0.00 (0.00)
Drug offense in 5 years prior	4510	0.02 (0.13)	0 - 1	0.02 (0.13)	0.02 (0.14)	0.00 (0.00)
Financial offense in 5 years prior	4510	0.02 (0.14)	0 - 1	0.02 (0.14)	0.02 (0.13)	0.00 (0.00)
Arrested between 1990 and 2006	4510	0.20 (0.40)	0 - 1	0.20 (0.40)	0.19 (0.39)	0.01 (0.01)
Neighborhood Characterestics						
Percent black in Census Tract	3633	51.4 (27.1)	0.7 - 94.8	51.1 (26.5)	51.8 (27.7)	-0.7 (0.9)
Percent Hispanic in Census Tract	3633	36.0 (21.4)	3.5 - 97.2	35.7 (21.0)	36.2 (21.8)	-0.6 (0.7)
Unemployment rate in Census Tract	3633	12.1 (5.5)	0 - 32.4	11.8 (5.4)	12.3 (5.6)	-0.4** (0.2)
Median household income in Census Tract	3633	33775 (12806)	9926 - 154375	33489 (12381)	34058 (13212)	-570 (425)
Poverty rate in Census Tract	3633	34.3 (15.9)	0 - 81.9	34.8 (15.7)	33.7 (16.1)	1.1** (0.5)
Crime rate	2938	135.1 (23.8)	76.1 - 165.5	134.3 (24.7)	135.8 (22.9)	-1.4 (0.9)
Violent crime rate	2938	13.4 (3.1)	6.7 - 16.9	13.3 (3.3)	13.5 (3.0)	-0.2* (0.1)
Property crime rate	2938	58.6 (10.7)	39.3 - 77.4	58.4 (10.8)	58.7 (10.7)	-0.4 (0.4)

Notes: Lottery numbers are classified as low or high based on if they are below or above the median (11896). Neighborhood crime rates are annual rates reported at the police division level from 2000 to 2005.
Significance: * 10% level; ** 5% level; *** 1% level

Table 3: Post-Lottery Descriptive Statistics [2010 Q1 to 2011 Q3]

	All		Low Lottery Numbers	High Lottery Numbers	Difference
	Mean (s.d.)	Range	Mean (s.d.)	Mean (s.d.)	Mean (s.d.)
Post voucher service	0.532 (0.499)	0 - 1	0.889 (0.314)	0.174 (0.379)	0.715*** (0.004)
Post lease-up with voucher	0.517 (0.500)	0 - 1	0.866 (0.341)	0.168 (0.374)	0.698*** (0.004)
Probability of arrest in a quarter	0.006 (0.079)	0 - 1	0.007 (0.084)	0.005 (0.074)	0.002* (0.001)
Probability of violent arrest in a quarter	0.001 (0.028)	0 - 1	0.001 (0.033)	0.000 (0.021)	0.001** (0.000)
Probability of drug arrest in a quarter	0.001 (0.033)	0 - 1	0.001 (0.036)	0.001 (0.030)	0.000 (0.000)
Probability of financial arrest in a quarter	0.001 (0.034)	0 - 1	0.001 (0.037)	0.001 (0.031)	0.000 (0.000)
Observations	31570		15785	15785	
Individuals	4510		2255	2255	

Notes: Lottery numbers are classified as low or high based on if they are below or above the median (11896). Unit of observation is a person-quater. Statistics are derived from all the quarters after 2009.
Significance: * 10% level; ** 5% level; *** 1% level

Table 4: Test of Randomization

		(1)	(2)
		Independent variables	
Dependent variables	Observations	Lottery number/1000	Quarter of voucher service
Arrested in 5 years prior to lottery	4510	0.000280	0.000327
		(0.000617)	(0.00127)
Violent offense in 5 years prior	4510	0.0000408	-0.000164
		(0.000305)	(0.000602)
Drug offense in 5 years prior	4510	0.000461	0.000907
		(0.000294)	(0.000596)
Financial offense in 5 years prior	4510	-0.0000880	-0.000367
		(0.000292)	(0.000618)
Number of arrests in 5 years prior	4510	0.000828	0.00164
		(0.000897)	(0.00180)
Number of violent arrests in 5 years prior	4510	0.000164	0.000111
		(0.000322)	(0.000640)
Number of drug arrests in 5 years prior	4510	0.000527	0.00112
		(0.000373)	(0.000755)
Number of financial arrests in 5 years prior	4510	0.000127	0.000167
		(0.000337)	(0.000721)
Arrested between 1990 and 2006	4510	0.000334	0.000505
		(0.000877)	(0.00179)
Age	4510	0.0109	0.0405
		(0.0312)	(0.0638)
Number of bedrooms	4510	0.00455**	0.00880**
		(0.00211)	(0.00428)
Male	3844	-0.000362	-0.00106
		(0.000701)	(0.00143)
Black	2612	0.000439	0.000930
		(0.000711)	(0.00147)
White	2612	-0.0000654	-0.0000336
		(0.000548)	(0.00112)
Other race	2612	-0.000373	-0.000896
		(0.000469)	(0.000986)
Homeless at the time of admission	2612	-0.0000769	-0.0000378
		(0.000122)	(0.000238)
Percent black in Census Tract	3633	0.0720	0.241*
		(0.0661)	(0.135)
Percent Hispanic in Census Tract	3633	0.0237	0.0105
		(0.0521)	(0.106)
Unemployment rate in Census Tract	3633	0.0287**	0.0758***
		(0.0136)	(0.0278)
Median household income in Census Tract	3633	24.34	58.21
		(31.22)	(63.59)
Poverty rate in Census Tract	3632	-0.0686*	-0.105
		(0.0392)	(0.0801)
Crimes per 1k population	2938	0.148**	0.406***
		(0.0652)	(0.136)
Violent crimes per 1k population	2938	0.0194**	0.0537***
		(0.00861)	(0.0179)
Property crimes per 1k population	2938	0.0428	0.109*
		(0.0291)	(0.0604)

Notes: Each cell represents a separate regression, estimating equation 1 with the observed covariates as the dependent variables. Unit of observation is an individual. Column 1 shows the coefficients of lottery number scaled down by 1000 and column 2 shows coefficients of the quarter in which the voucher is serviced. Robust standard errors are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 5: First stage - Relationship between Voucher Service and Lease-Up

	All		Males	Females
	(1)	(2)	(3)	(4)
	Post lease-up with voucher			
Post voucher service	0.849***	0.849***	0.855***	0.845***
	(0.00394)	(0.00394)	(0.0135)	(0.00475)
Observations	85690	85690	7106	61693
Individuals	4510	4510	374	3247
Quarter FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes

Notes: Each column represents a separate regression estimating equation 2 with the indicator for post lease-up as the dependent variable. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 6: Effect of Vouchers on Crime - By Gender and Crime Type

	All			Males			Females		
	Mean	(1)	(2)	Mean	(3)	(4)	Mean	(5)	(6)
Panel A: OLS									
All Arrests	0.0055	0.000487	0.000505	0.0174	-0.000247	-0.00181	0.0039	-0.000306	-0.000302
		(0.000975)	(0.000970)		(0.00461)	(0.00433)		(0.000984)	(0.000987)
Violent Arrests	0.0007	0.000685**	0.000661*	0.0013	0.00392*	0.00384*	0.0005	-0.0000387	-0.0000865
		(0.000349)	(0.000348)		(0.00220)	(0.00212)		(0.000311)	(0.000313)
Drug Arrests	0.0012	0.0000780	0.000230	0.0060	-0.00162	-0.00131	0.0008	-0.00000129	0.000109
		(0.000384)	(0.000382)		(0.00211)	(0.00205)		(0.000384)	(0.000381)
Financial Arrests	0.0007	0.000191	0.000136	0.0007	-0.00134	-0.00145	0.0006	0.000454	0.000424
		(0.000427)	(0.000424)		(0.00156)	(0.00147)		(0.000454)	(0.000456)
Panel B: Negative Binomial									
All Arrests		0.0758	0.0765		-0.0200	-0.155		-0.0585	-0.0750
		(0.151)	(0.152)		(0.373)	(0.346)		(0.188)	(0.190)
Violent Arrests		0.787**	0.772**		1.696**	1.566**		-0.0655	-0.135
		(0.376)	(0.387)		(0.820)	(0.795)		(0.528)	(0.536)
Drug Arrests		0.0766	0.231		-0.411	-0.396		-0.00198	0.196
		(0.374)	(0.372)		(0.550)	(0.543)		(0.577)	(0.563)
Financial Arrests		0.149	0.0595		-1.073	-1.082		0.417	0.333
		(0.330)	(0.331)		(1.340)	(1.162)		(0.410)	(0.420)
Observations		85690	85690		7106	7106		61693	61693
Individuals		4510	4510		374	374		3247	3247
Quarter FE		Yes	Yes		Yes	Yes		Yes	Yes
Controls		No	Yes		No	Yes		No	Yes

Notes: The first column for each group presents the Pre-Lottery Mean which is the mean of quarterly probability of arrest in the crime category from the year 2006. Each cell in the numbered columns represents a separate regression estimating equation 2 without and with controls in the odd and even columns respectively. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table 7: Effect of Voucher Service on Crime - By time since Voucher Service

	All	Males	Females
	(1)	(2)	(3)
Panel A: All Arrests			
Pre-Lottery Mean	0.0055	0.0174	0.0039
< 1 yr since voucher service	0.00109	0.000585	0.000123
	(0.00104)	(0.00421)	(0.00110)
> 1 yr since voucher service	-0.000584	-0.00623	-0.00109
	(0.00128)	(0.00665)	(0.00130)
Panel B: Violent Arrests			
Pre-Lottery Mean	0.0007	0.0013	0.0005
< 1 yr since voucher service	0.000728**	0.00325*	-0.0000689
	(0.000360)	(0.00186)	(0.000323)
> 1 yr since voucher service	0.000537	0.00492	-0.000119
	(0.000475)	(0.00324)	(0.000459)
Panel C: Drug Arrests			
Pre-Lottery Mean	0.0012	0.0060	0.0008
< 1 yr since voucher service	0.000372	-0.000422	0.000177
	(0.000416)	(0.00230)	(0.000416)
> 1 yr since voucher service	-0.0000339	-0.00295	-0.0000173
	(0.000510)	(0.00307)	(0.000490)
Panel D: Financial Arrests			
Pre-Lottery Mean	0.0007	0.0007	0.0006
< 1 yr since voucher service	0.000257	-0.00129	0.000522
	(0.000496)	(0.00162)	(0.000546)
> 1 yr since voucher service	-0.0000894	-0.00175	0.000243
	(0.000455)	(0.00146)	(0.000459)
Observations	85690	7106	61693
Individuals	4510	374	3247
Quarter FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: Each column within a panel represents a separate regression estimating a version of equation 2 with the independent variable split up by duration since voucher service. Pre-Lottery Mean is the mean of quarterly probability of arrest in the crime category from the year 2006. Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses. Significance: * 10% level; ** 5% level; *** 1% level

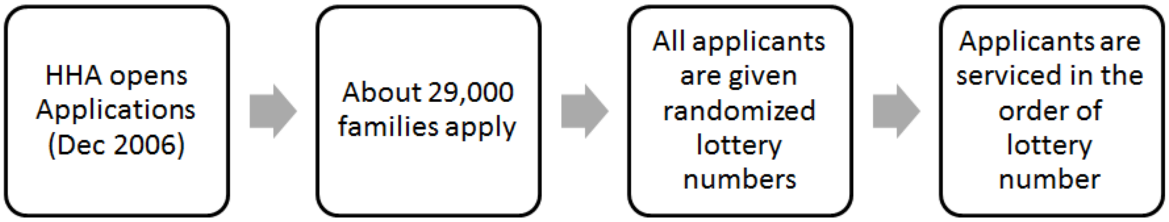
Table 8: Test of Differential Attrition across Lottery Numbers - Registration and Voting in 2012

	All		Males		Females	
	(1) Registered	(2) Voted	(3) Registered	(4) Voted	(5) Registered	(6) Voted
Panel A						
Lottery number/1000	0.000520	-0.0000686	0.00277	0.00235	-0.000800	-0.000137
	(0.00102)	(0.00103)	(0.00355)	(0.00356)	(0.00121)	(0.00123)
Panel B						
Quarter of voucher service	0.000521	-0.000601	0.00694	0.00508	-0.00248	-0.000885
	(0.00208)	(0.00211)	(0.00718)	(0.00733)	(0.00245)	(0.00251)
Observations	4510	4510	374	374	3247	3247

Notes: Each cell represents a separate regression, estimating equation 1 with dummy indicating being registered in 2012 as the dependent variable in the odd columns and a dummy indicating having voted in 2012 as the dependent variable in the even columns. Unit of observation is an individual. Panel A shows the coefficients for lottery number scaled down by 1000 and Panel B shows coefficients for the voucher service quarter. Robust standard errors are presented in parentheses.

Figure 1: Lottery and Voucher Service Processes

(a) Lottery Process



(b) Voucher Service Process

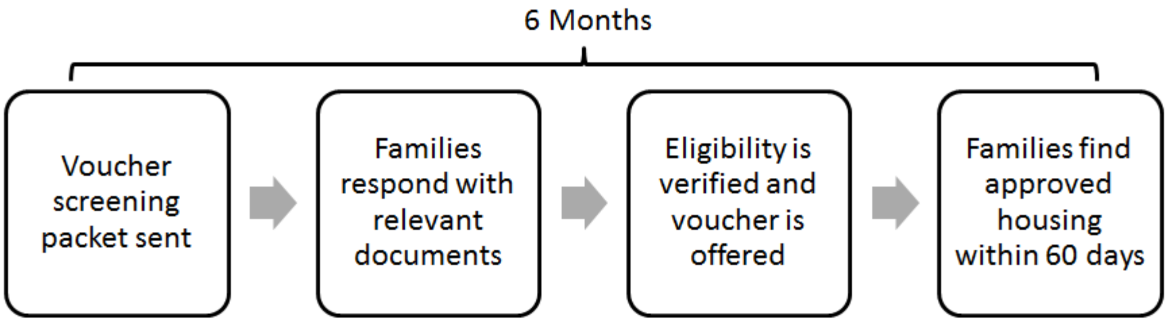
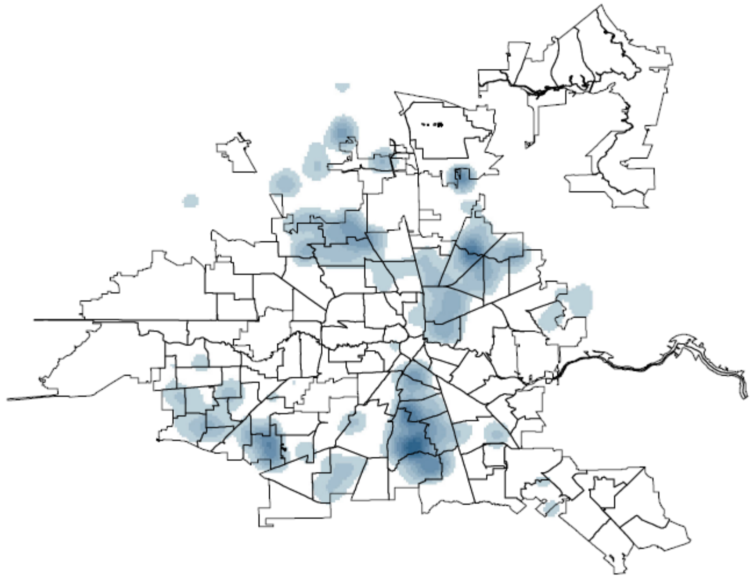
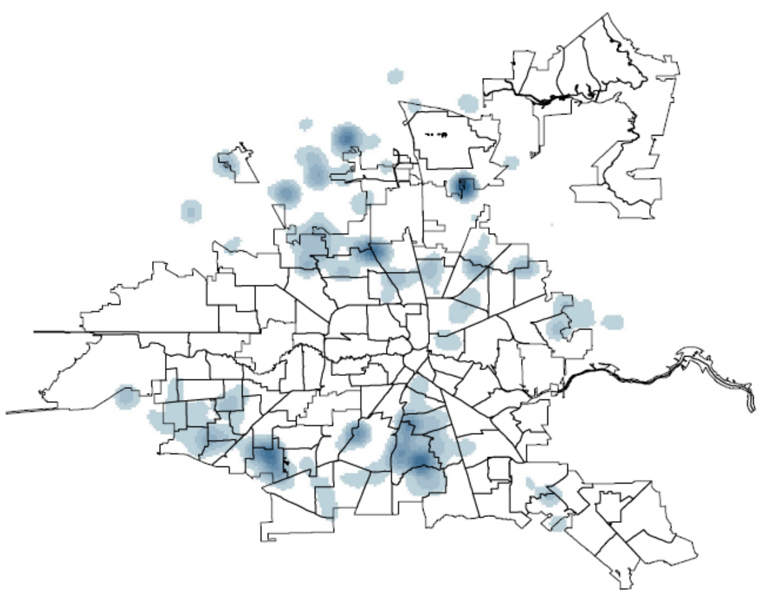


Figure 2: Heatmaps of Application and Voucher Use Addresses

(a) Distribution of Application Addresses

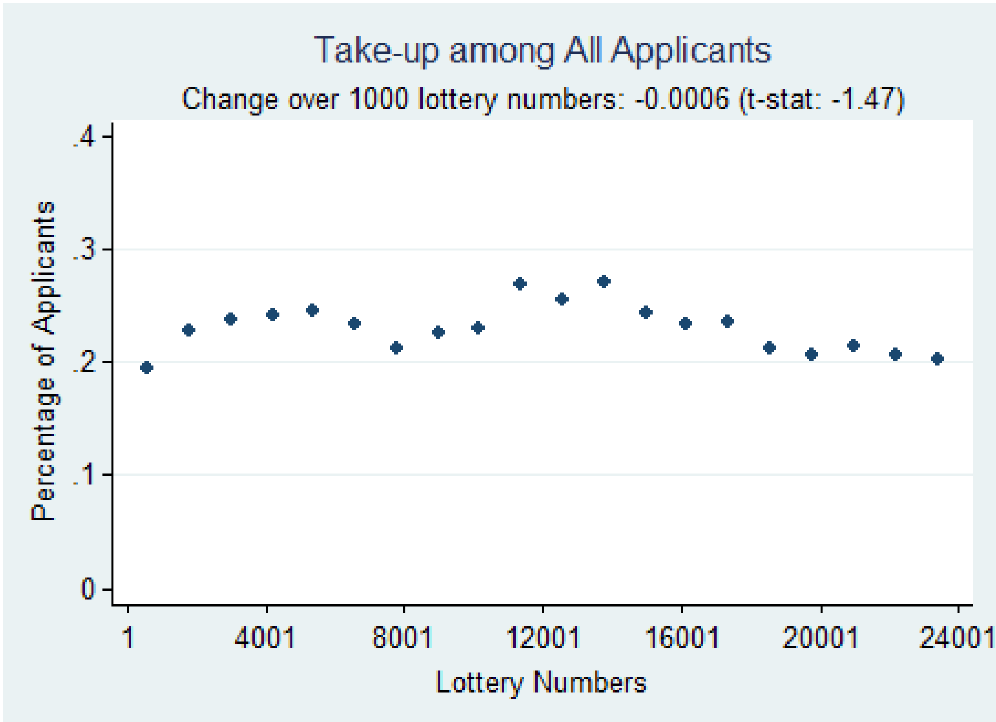


(b) Distribution of Voucher Use Addresses



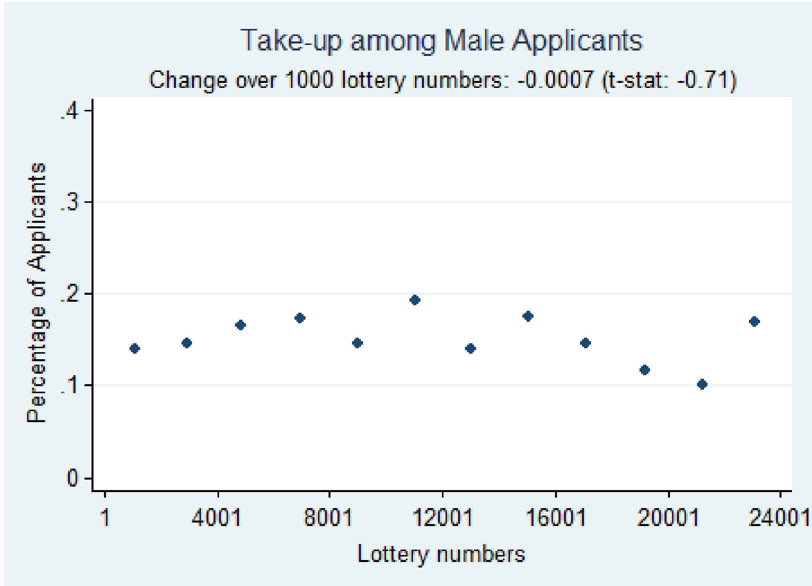
Notes: The heat maps are created in ArcMap using a point density operation that creates a grid over the map and then counts the number of address points within each grid cell. The outline indicates the Houston Police Department districts.

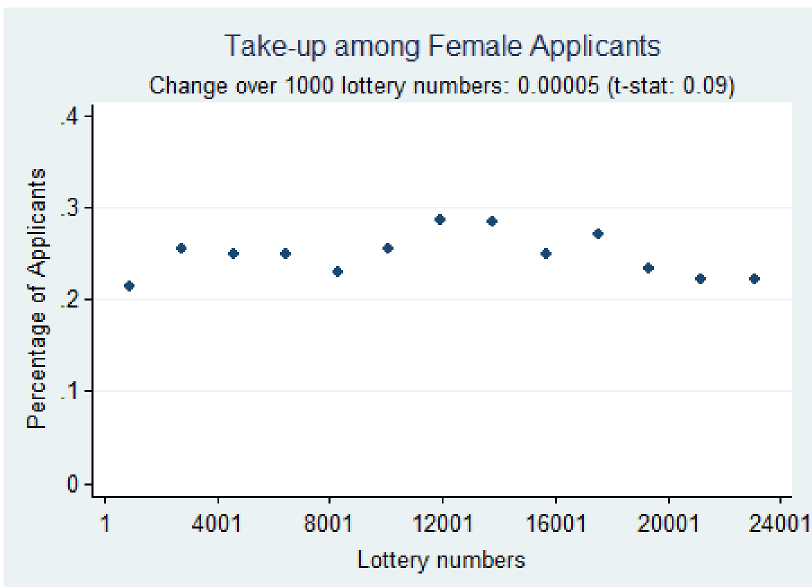
Figure 3: Take-up Rates across Lottery Numbers



Notes: Each bubble represents the percentage of take-up within bins of about 980 individuals.

Figure 4: Take-up Rates by Gender

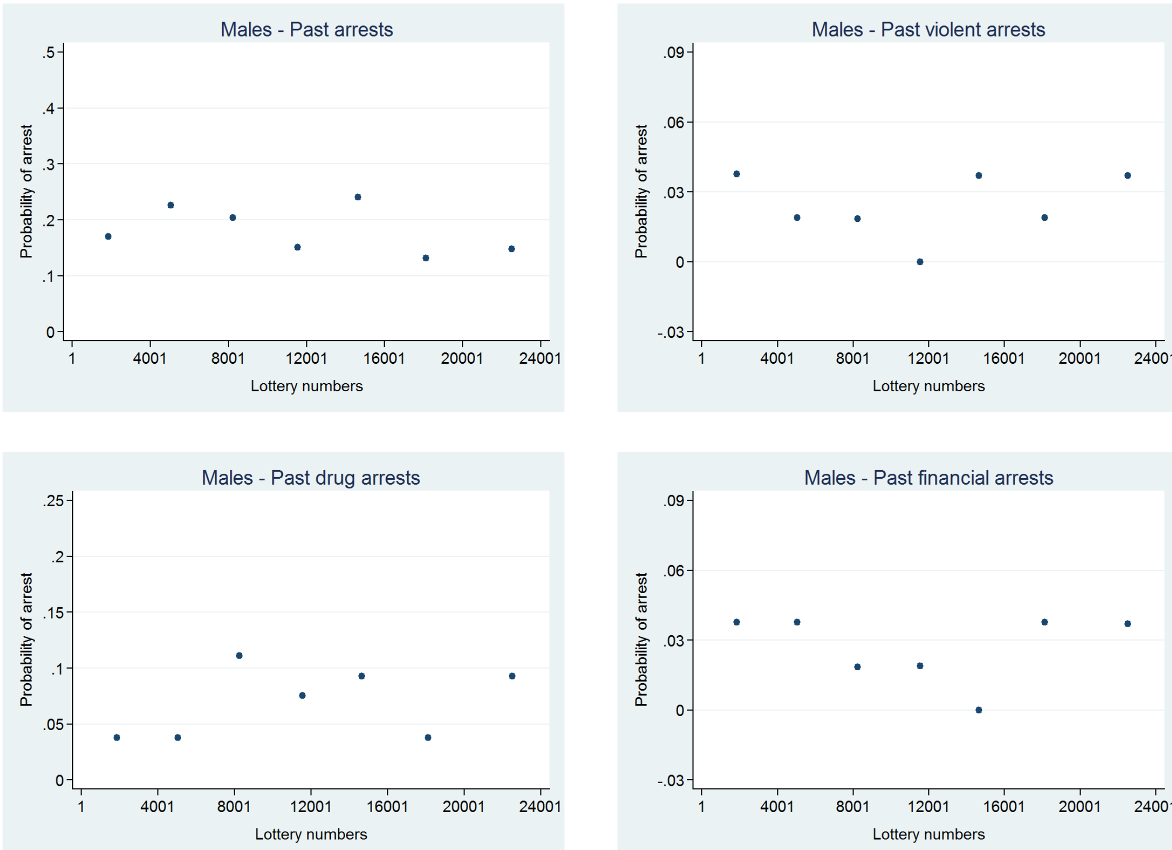




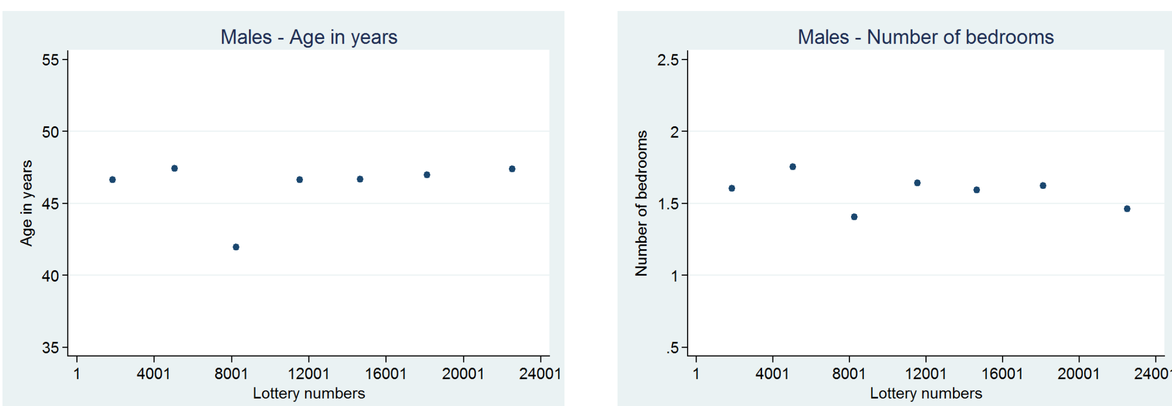
Notes: Each bubble represents the percentage of take-up within bins of about 200 men and about 1000 women respectively.

Figure 5: Test of Randomization: Distribution of Pre-Lottery Characteristics for Males

(a) Crime History



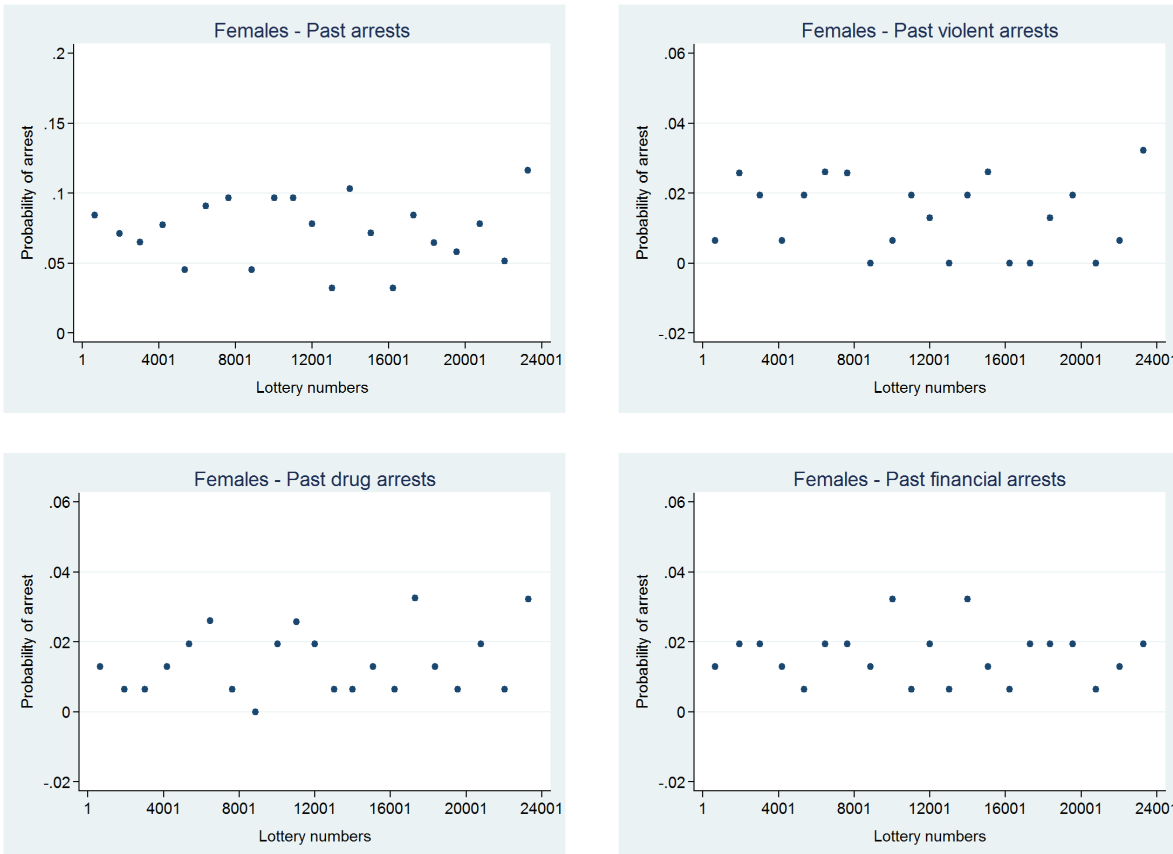
(b) Demographics



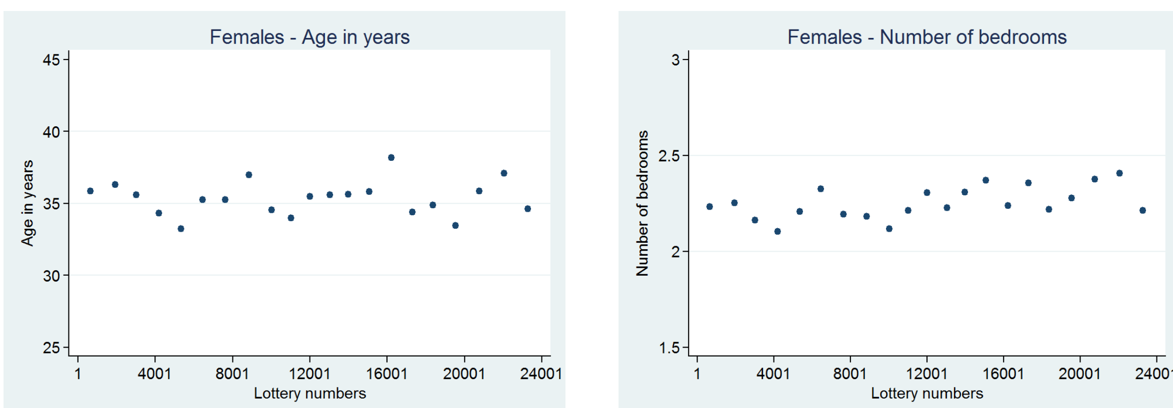
Notes: Each bubble represents the local average of the variable within bins of 53-54 men. Crime history variables represent the probability of arrest in the crime category between 2002 and 2006.

Figure 6: Test of Randomization: Distribution of Pre-Lottery Characteristics for Females

(a) Crime History

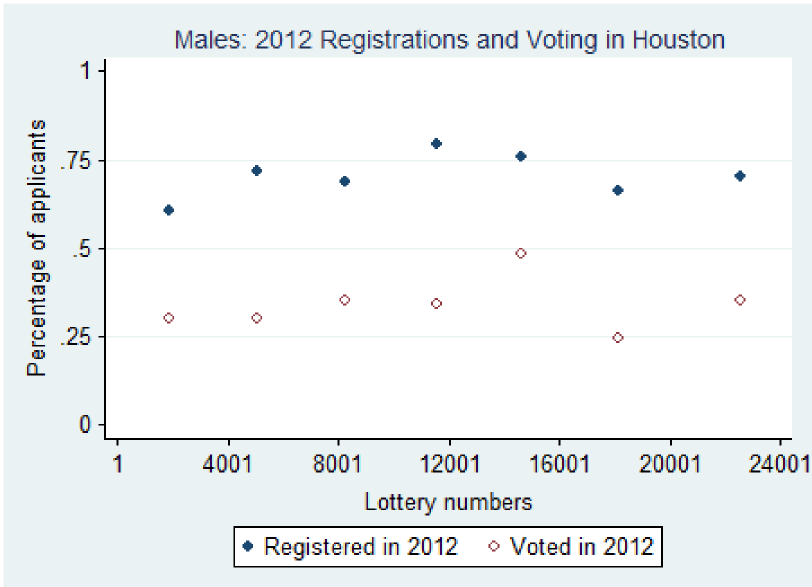


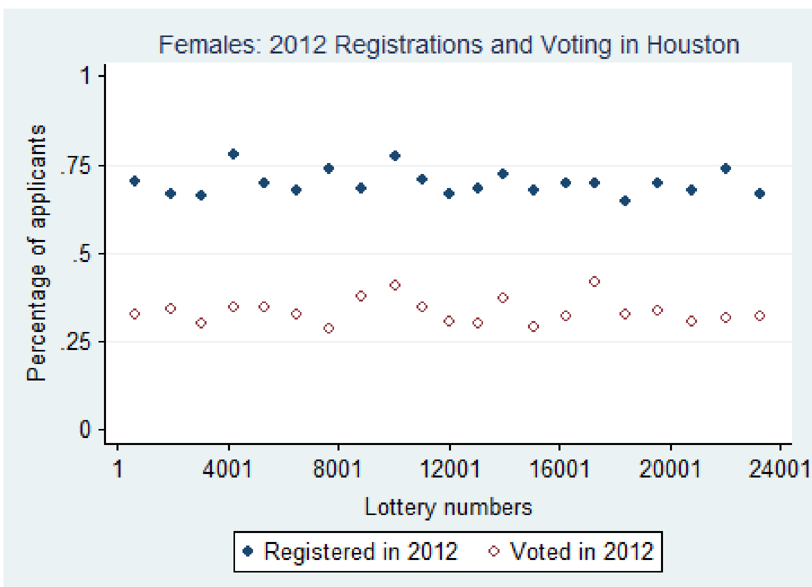
(b) Demographics



Notes: Each bubble represents the local average of the variable within bins of 154-155 women. Crime history variables represent the probability of arrest in the crime category between 2002 and 2006.

Figure 7: Test for Attrition - Likelihood of Voter Registration and Voting in Houston in 2012 across Lottery Numbers





Notes: Each bubble represents the local percentage within bins of 53-54 men and 154-155 women respectively, of individuals who were registered to vote and who voted in Houston in 2012.

APPENDIX

Table A1: Classification of crimes into categories

Category	**Included crimes**
Violent	Assault, Aggravated Assault, Arson, Kidnapping, Murder, Robbery, Sexual Assault
Drug	Alcohol related offenses, DUI, Manufacture, Possession or Sale of contraband products
Financial	Auto Theft, Burglary, Gambling, Robbery, Shoplifting, Theft, White Collar crimes (Forgery, Fraud etc.)
Unclassified	Minor traffic offenses, Carrying/Discharging prohibited weapons, Criminal Mischief, Criminal Trespassing, Evading arrest, Indecent behavior/exposure, Prostitution related arrests

Table A2: Intent to treat estimates with controls and leads

	All			Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: All Arrests									
Post voucher service	0.000487	0.000505	0.000689	-0.000247	-0.00181	-0.000664	-0.000306	-0.000302	-0.000635
	(0.000975)	(0.000970)	(0.00111)	(0.00461)	(0.00433)	(0.00516)	(0.000984)	(0.000987)	(0.00113)
Announcement effect			0.000358			0.00672			-0.000981
			(0.00122)			(0.00651)			(0.00126)
Lead			0.000295			-0.00357			-0.0001000
			(0.00106)			(0.00550)			(0.00109)
Panel B: Violent Arrests									
Post voucher service	0.000685**	0.000661*	0.000874**	0.00392*	0.00384*	0.00478**	-0.0000387	-0.0000865	0.0000894
	(0.000349)	(0.000348)	(0.000391)	(0.00220)	(0.00212)	(0.00214)	(0.000311)	(0.000313)	(0.000345)
Announcement effect			0.000761*			0.00286			0.000671
			(0.000432)			(0.00240)			(0.000464)
Lead			-0.000102			0.000438			-0.000142
			(0.000367)			(0.00197)			(0.000326)
Panel C: Drug Arrests									
Post voucher service	0.0000780	0.000230	0.000657	-0.00162	-0.00131	0.00261	-0.00000129	0.000109	0.000230
	(0.000384)	(0.000382)	(0.000447)	(0.00211)	(0.00205)	(0.00227)	(0.000384)	(0.000381)	(0.000456)
Announcement effect			0.000994*			0.0102**			0.000000596
			(0.000558)			(0.00416)			(0.000495)
Lead			0.000473			0.00407			0.000493
			(0.000473)			(0.00363)			(0.000477)
Panel D: Financial Arrests									
Post voucher service	0.000191	0.000136	0.000418	-0.00134	-0.00145	-0.00112	0.000454	0.000424	0.000640
	(0.000427)	(0.000424)	(0.000460)	(0.00156)	(0.00147)	(0.00174)	(0.000454)	(0.000456)	(0.000481)
Announcement effect			0.000457			0.000840			0.000182
			(0.000476)			(0.00176)			(0.000453)
Lead			0.000569			0.000391			0.000648
			(0.000496)			(0.00187)			(0.000568)
Observations	85690	85690	85690	7106	7106	7106	61693	61693	61693
Individuals	4510	4510	4510	374	374	374	3247	3247	3247
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Notes: Each column in each panel represents a separate regression. Columns 3, 6 and 9 present results from estimating equation 2 with indicators for 1-2 quarters before voucher service (announcement effecnt) and 3-4 quarters before voucher service (leads testing for pre-treatment trends). Controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level

Table A3: Intent to treat estimates with controls for neighborhood characteristics

	All			Males			Females		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
All Arrests	0.000505	0.000531	0.000603	-0.00181	-0.00220	-0.00215	-0.000302	-0.000223	-0.000153
	(0.000970)	(0.000969)	(0.000971)	(0.00433)	(0.00437)	(0.00440)	(0.000987)	(0.000987)	(0.000989)
Violent Arrests	0.000661*	0.000652*	0.000666*	0.00384*	0.00376*	0.00381*	-0.0000865	-0.000104	-0.0000910
	(0.000348)	(0.000348)	(0.000351)	(0.00212)	(0.00213)	(0.00214)	(0.000313)	(0.000313)	(0.000315)
Drug Arrests	0.000230	0.000258	0.000293	-0.00131	-0.00130	-0.00106	0.000109	0.000139	0.000156
	(0.000382)	(0.000383)	(0.000383)	(0.00205)	(0.00202)	(0.00201)	(0.000381)	(0.000384)	(0.000384)
Financial Arrests	0.000136	0.000162	0.000184	-0.00145	-0.00142	-0.00148	0.000424	0.000466	0.000485
	(0.000424)	(0.000424)	(0.000427)	(0.00147)	(0.00148)	(0.00151)	(0.000456)	(0.000456)	(0.000461)
Observations	85690	85690	85690	7106	7106	7106	61693	61693	61693
Individuals	4510	4510	4510	374	374	374	3247	3247	3247
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Dummy for missing demographic controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Crime controls	No	No	Yes	No	No	Yes	No	No	Yes
Dummy for missing crime controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: Each cell represents a separate regression from estimating equation 2 with a different set of control variables. Main controls include age at the time of the lottery, number of bedrooms and a dummy indicating arrest in the crime category in the 5 years prior to the lottery. Demographic controls include percent black, percent Hispanic, unemployment rate, median household income and poverty rate for the census tract of the individual's application address. Crime controls include rates for overall crime, violent and property crimes per 1000 people in the police division of the individual's application address. To maintain the number of observations constant across specifications, we include dummy variables indicating whether the demographic or crime controls are missing. Unit of observation is a person-quarter. Robust standard errors, clustered at the individual level, are presented in parentheses.
Significance: * 10% level; ** 5% level; *** 1% level